PAYMENT POLICY AND INEFFICIENT BENEFITS IN THE MEDICARE+CHOICE PROGRAM

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Abstract

We investigated whether zero premium health plans in the M+C program are likely to offer inefficient benefits. We took advantage of a natural experiment to minimize variation in the cost of providing coverage to beneficiaries (a quantity that is notoriously hard to measure and has been changing rapidly). Our findings indicate that benefits in zero premium plans were more sensitive to changes in payment rates relative to plans that charged nonzero premiums. These results strongly suggest that the current Medicare policy that forbids premium rebates effectively forces plans to offer benefits they do not believe are valued by enrollees at or above their cost.

Introduction

The Medicare + Choice program (M+C) currently provides health insurance coverage to 5 million Medicare beneficiaries through privately operated managed care plans (CMS, 2002). In exchange for accepting some limits on utilization and choice of provider, M+C enrollees typically receive more extensive coverage than they would under traditional fee-for-service Medicare. Until recently, a substantial fraction of M+C enrollees received outpatient prescription drug coverage and paid either nothing or a small additional premium for their coverage. Starting in 2000, however, the program began to experience profound changes. Plans began to withdraw from a substantial number of markets, leaving enrollees to search for coverage elsewhere. In January 2001, over 150,000 Medicare beneficiaries previously enrolled in M+C were left with no M+C plans doing business in their counties (HCFA, 2000). In addition to the market withdrawals, plans began to increase premiums and reduce benefits in their remaining markets (Gold, 2001). In this climate, any opportunity to improve the value delivered to beneficiaries without increasing the cost of coverage should be of interest to Congress and the Centers for Medicare and Medicaid Services (CMS).

One such opportunity may exist in the way M+C benefits are determined. M+C plans are required to calculate the cost of providing core benefits to enrollees and then show that any payments they receive above this cost will be either returned to the government or spent on additional benefits and lower premiums. Since plans have not been known to return money, the effect of this requirement is to force plans doing business in high payment rate counties to offer additional benefits or lower premiums. As long as plans are charging a variety of nonzero premiums, there is minimal loss of

efficiency from this mechanism. Beneficiaries who highly value certain benefits can search for a plan that offers those benefits and pay the marginal premium that corresponds to their choice, and the fact that average premiums do not cover costs represents a simple income transfer from taxpayers to Medicare beneficiaries. By contrast, the loss of efficiency may be more substantial when plans charge zero premiums. Under current rules, M+C plans may not charge premiums less than zero (i.e., premium rebates), so zero premium plans in high payment counties may be forced to offer benefits that their enrollees would not purchase if faced with the true marginal cost. Consequently, the rule that forbids rebates can be thought of as a constraint, limiting plans' ability to optimize the attractiveness of their products in a competitive environment (Feldman et al., 1993). Because their freedom to adjust premium has been constrained, we predict that the benefits offered by zero premium plans should be more sensitive to changes in payment rates than the benefits offered by plans charging nonzero premiums. This should be true simply because some of the marginal payment can be used to reduce premiums if the constraint is not binding, whereas the entire amount must be used to increase benefits if premiums cannot be adjusted.

To evaluate our prediction, we take advantage of a natural experiment created by the Benefits Improvement and Protection Act of 2000 (BIPA), which changed payment rates just after the rates for 2001 went into effect in January. In response to BIPA, a special set of adjustments to benefits and premiums were permitted between January and March of 2001, off the usual annual schedule. We use data generated by this event to model the relationship between payment rate changes and adjustments in benefits offered by M+C plans, holding underlying costs nearly constant, and then test whether this

relationship differed systematically between plans charging zero and nonzero premiums. Based on the results of these tests, we estimate endogenous switching models to account for the simultaneous choice of premium level and generosity of benefits.

Although other studies have attempted to describe M+C plan behavior (Gold, 2001; MedPAC, 2000; GAO, 2000), to our knowledge there have been no empirical studies that address the question of inefficient benefits, and only one (Pizer and Frakt, 2002) that takes advantage of the natural experiment resulting from BIPA. Our data and statistical models are similar to those used by Pizer and Frakt, adapted to the purposes of this study.

We find that benefits offered by plans that do not charge a premium are more sensitive to payment changes than are benefits offered by plans that charge nonzero premiums. Zero premium plans responded to payment increases by increasing caps on prescription drug coverage, reducing copayments for generic drugs, and reducing copayments for brand-name drugs, all by larger margins than did their nonzero premium counterparts.

Our findings suggest that the M+C program currently forces zero premium plans to offer some benefits that cost more to provide than the plans' estimates of their value to beneficiaries. Therefore, the program would provide better value for society if payments were set such that all plans charged nonzero premiums, but this would be very difficult politically. Alternatively, the same efficiency could be achieved if rebates were permitted. Though CMS' plan to allow premium rebates in 2003 is controversial (see Feldman, et al., 2001), this study indicates that rebates are the most practical means to ensure efficiency of benefits in the Medicare + Choice program.

Theoretical Framework

Our theory is derived from Sherwin Rosen's (1974) path breaking analysis of product differentiation in pure competition. Suppose that M+C enrollees purchase a plan "benefit index" consisting of membership in an M+C plan with varying levels of benefits. Examples of such benefits are coverage of Medicare's deductibles, coinsurance, and services like outpatient prescription drugs that are not included in standard Medicare.

Let a consumer with income y have a utility function U(x,q), where x is all other goods consumed and q is the benefit index. Setting the price of x equal to one dollar and maximizing utility with respect to $y = p(q) + x$, we have the solution that $U_q/U_x = p'(q)$, where p(q) is the out-of-pocket premium and the first derivative $p'(q)$ is the price of the benefit index.

Following Rosen, we define expenditure functions, $\theta = \theta(q,y)$ that represent the beneficiary's willingness to pay for different levels of benefits, given their income. Then $U = U(y-\theta,q)$ can be differentiated to obtain $U_q/U_x = \theta'(q)$, the slope of an indifference curve between the benefit index and the willingness to pay. At equilibrium, $\theta'(q) = p'(q)$. By further differentiation, $\theta_{qy} = X_y(U_xU_{qx} - U_qU_{xx})/U_x^2$. The numerator of this expression determines the sign of the income effect for good q in standard theory. If $(U_xU_{qx} - U_qU_{xx}) = 0$, the family of expenditure functions is parallel, as shown in Figure 1. If benefits are a normal good, the indifference curves become steeper as y increases.

The next step in our theory requires a brief description of how the government pays M+C health plans. The government determines how much it will pay for each enrollee by using a mixture of administrative and political calculations. These calculations are related to the plans' costs but they also involve a considerable degree of

exogenous price setting (e.g., Congress has authorized higher payments for rural areas in order to encourage M+C plans to enter rural counties). Consequently, an *exogenous* increase in government payments in competitive markets will lead to lower premiums for enrollees. The p(q) line in Figure 1 shifts downward, as it would if the enrollee's income increased. Denoting the government payment by "ρ," then $dq/dy = dq/d\rho > 0$ if benefits are a normal good.

As the government payment increases, the enrollee's out-of-pocket premium will be driven to zero, as it was for 60% of all M+C enrollees in 1999. Once the out-of-pocket premium is driven to zero, further increases in ρ can only lead to increases in benefits. The enrollee in Figure 1 would like to have a negative premium at point A, but he/she must settle for a zero premium and more benefits at point B. We refer to benefit index B as "inefficient," because the enrollee could have the same level of utility at a lower total premium cost. A monetary measure of the welfare loss from inefficient benefits is distance Δ: payments to the M+C plans could be cut by that amount with no loss of welfare.

Statistical Framework

We start with a basic statistical model of plans' benefit and premium decisions. Next, we describe tests to determine whether plan decisions were systematically different, depending on premium status. Finally, we modify the original framework to account for differences in plan behavior by premium status using switching regressions. Only this last set of models will permit us to evaluate whether zero premium plans were more sensitive to payment changes than plans that charge premiums.

Basic framework

Because we are considering the possibility that premium status affects the influence of payment rates on benefit decisions, it is convenient to use separate equations to model plans' premium decisions and their benefit decisions, even though the covariates in both equations are almost the same. Our model can be written as:

$$(1) \quad \text{premium}_t^{p,c} = \beta_1 \text{payment}_t^c + \beta_2 \text{march}_t + \beta_3 \text{supply}_t^c + \beta_4 \text{demand}_t^c + \beta_5 \text{competition}_{t-1}^{p,c} + \delta^p + \varepsilon_{1t}^{p,c}$$

$$(2) \quad \text{benefit}_t^{p,c} = \beta_1 \text{payment}_t^c + \beta_2 \text{march}_t + \beta_3 \text{supply}_t^c + \beta_4 \text{demand}_t^c + \beta_5 \text{competition}_{t-1}^{p,c} + \beta_6 \lambda_t^{p,c} + \delta^p + \varepsilon_{2t}^{p,c}$$

where t indexes the benefit period, p is a plan index, and c is a county index; $\text{benefit}_t^{p,c}$ denotes a particular continuous benefit or cost-sharing variable (generic copayment amount, brand-name copayment amount, or cap on prescription drug benefit); $\text{premium}_t^{p,c}$ is a binary variable indicating whether premium was greater than zero; payment_t^c represents the government's base payment rate; march_t is an indicator of the benefit period (0 for January 2001 and 1 for March 2001); $\text{supply}_t^{p,c}$ is a vector of variables thought to affect plans' supply decisions; $\text{demand}_t^{p,c}$ is a similar vector thought to affect beneficiaries' demand decisions; $\text{competition}_{t-1}^{p,c}$ is a vector of variables thought to affect the intensity of competition facing each plan; $\beta_1, \beta_2, \beta_3, \beta_4, \beta_5, \beta_6$ are coefficients to be estimated, δ^p denotes a plan-level fixed effect, and $\varepsilon_{1t}^{p,c}$ and $\varepsilon_{2t}^{p,c}$ are the residuals. $\lambda_t^{p,c}$ denotes the expected value of $\varepsilon_{1t}^{p,c}$, conditional on the outcome of Equation (1). This term is only included in our switching regression specifications, discussed below.

Plan-level fixed effects were included in the specification because we suspected that benefit and premium decisions were not made independently at the county level, despite the fact that payment rates varied by county. There are two reasons why plan effects are likely to have been important: first, the administrative complexity of obtaining approval and managing different benefit and premium packages by county would have been burdensome, and, second, it would have been difficult for plans to explain to enrollees why premiums and benefits might be different across seemingly arbitrary county lines.

The vector of supply variables contained elements reflecting variation in input prices, bargaining power, capital intensity, and practice patterns. Permanent geographic variation in input prices was measured by historical per capita Medicare Part A spending (as in Wholey et al., 1995). Bargaining power is thought to vary with the number of physicians per capita (Wholey et al., 1993) and urban/adjacent/rural status (McBride, 1998). HMOs should have stronger bargaining positions in relatively urban counties with high numbers of physicians per capita because under these circumstances it is easier for plans to direct beneficiaries to preferred providers (because there are more providers to choose from and traveling distance is minimal). Plans' marginal costs should also vary with capital intensity, measured in our models by the per capita number of hospital beds in the county. Higher numbers of hospital beds per capita are thought to be associated with higher marginal costs because of the cost of maintaining additional beds (Gaynor and Anderson, 1995) and potentially as a reflection of regional practice patterns (Knickman and Foltz, 1985). Hospital utilization patterns also underlie the effects of PIP-DCG scores in our model because these risk scores rely on inpatient diagnoses and

our specifications included historical Medicare Part A per capita spending to control for differences in input prices. Thus, when comparing two counties with the same input prices but different risk scores, the county with a higher risk score should have a practice pattern that relies more heavily on inpatient hospitalizations. Although this differs from the most common interpretation of the PIP-DCG risk score as a measure of average health status at the county level, it is appropriate in a model that also contains per capita Part A spending.

Plan decisions will also be affected by variations in the elasticity of demand for health insurance. The demand vector in our models included per capita county income because the desire to avoid financial risk should vary with personal resources (Cutler and Zeckhauser, 2000). Furthermore, Nyman (1998) argued that health insurance is valuable to its consumers primarily because it makes potentially needed procedures affordable. This motive would vary with personal resources as well. In addition to income, our models included the fraction of the population that is over 65 years old because markets with high concentrations of elderly residents may have more rapid exchanges of information among the elderly and therefore individual plans might face more elastic demand.

Although arguably another component of demand, we chose to highlight competition separately for clarity of presentation. The competition vector included the Herfindahl index (a measure of industry concentration),[1] and a variable reflecting the benefits offered by other plans in the county in the previous period. Higher industry concentration is expected to facilitate collusion, resulting in higher profits (Schmalensee, 1989) and therefore less generous benefits. The second competition variable depended

on the model being estimated; for example, in a generic copayment regression it was the average generic copayment charged by other plans in the county. Both this "other benefits" variable and the Herfindahl index were constructed using data from 2000, one year prior to the study period. We employed this time lag primarily because plans' benefit decisions would have been made in the prior period and filed with CMS before going into effect. Additionally, this specification has the benefit of reducing any potential endogeneity that might have been introduced by including the contemporaneous versions of these variables. It should be noted that by including both the Herfindahl index and variables reflecting other plans' decisions in each model, we estimated the effect of industry concentration holding lagged competitors' decisions constant and *vice versa*. Ordinary least squares can be used to estimate Equation (2), and probit methods can be used for Equation (1). For all models, observations were weighted by the number of enrollees in each plan-county-period so that smaller plans were given less weight and larger plans more weight.[2]

Testing and accounting for differences in plan decisions

Using this framework, we approached the question of inefficient benefits by asking whether the constraint that prevented plans from offering premium rebates caused them to modify their responses to payment changes. For testing purposes, our null hypothesis was that the premium constraint did not matter. If the null hypothesis were true, the outcome of Equation (1) would have no impact on Equation (2). Therefore we could test the null hypothesis by comparing results from Equation (2) when estimated on the entire sample to results obtained from a sample restricted to zero premium plan-county-periods. If the coefficients on the independent variables were significantly

different between the two specifications (as determined by a Hausman test), then we could reject the null hypothesis.[3] Since we had three different benefit models, we could perform three different Hausman tests for robustness.

If the Hausman tests rejected the null hypothesis, then plan benefit decisions were different depending on premium status. To take this dependency into account, Equations (1) and (2) could be estimated jointly, allowing the residual terms, $\varepsilon_{1t}^{p,c}$ and $\varepsilon_{2t}^{p,c}$, to be correlated with each other in an endogenous switching model. Since the switching model produced two sets of estimates for Equation (2), depending on premium status, we could use these results to test our prediction that constrained plans would be forced to offer more generous benefits than unconstrained plans when faced with the same change in payment rates.

We followed a two-step method, estimating Equation (1) as the first step and using the results to formulate the expected value of $\varepsilon_{1t}^{p,c}$, conditional on the outcome of Equation (1). The second step was to include that expected value in two separate specifications of Equation (2), one for zero premium observations and one for nonzero premium observations (Maddala, 1983). To take the stochastic nature of Equation (1) into account when calculating standard errors for Equation (2), we bootstrapped standard errors for the second step (Efron, 1993).

Data

To measure benefits offered by Medicare risk plans, we obtained data from CMS' Medicare Compare database. To measure urban/rural status, payment rates, and other county characteristics that might be associated with cost of coverage, we combined data from several sources including the 2000 Area Resource File (ARF), CMS'

State/County/Plan Files, and county-level average Principal In-Patient Diagnostic Cost Groups (PIP-DCG) risk scores calculated by CMS. In this section we provide details on the construction of the analytic files and the characteristics of the data.

File Construction

Two datasets were constructed, one for January 2001 (before adjustment to BIPA) and one for March 2001 (after adjustment to BIPA). We started with 352 plan records from January Medicare Compare and 359 from March. Merging to the Service Area File, the State/County/Plan File, the Area Resource File, and the Risk Score data we obtained 1,159 matched plan-counties for January and 1,171 for March. Finally, we dropped non Medicare + Choice plans and plans with missing or zero enrollment[4], yielding 1,132 and 1,136 plan-county records for January and March, respectively.

The fact that zero premium plans offer less generous benefits than plans charging premiums is evident from Table 1, which contains means and numbers of observations for a series of benefits by zero and nonzero premium status. Copayments for both generic and brand name drugs were higher and caps on drug benefits were lower for zero premium plans. We chose to focus on these three benefits because they are among the most important to beneficiaries (MedPAC, 2000) and because they are continuous (in contrast to binary). Continuous variables are necessary because our switching models are only identified for plans with variation in the dependent variable *within* premium status. Consequently, sample sizes for binary dependent variables (e.g., dental coverage) were unacceptably small.[5]

Although the fact that zero premium plans were less generous at first might seem to suggest that our prediction is incorrect, it might also simply reflect geographic

variation in the ability and willingness to pay for benefits. To truly judge the accuracy of our prediction, we will have to contrast the relationship between payments and benefits by premium status in a multivariate context.

Results

The question of whether premium status affects plan benefit decisions was answered in the affirmative by the Hausman test results reported in Table 2. All three tests strongly rejected the null hypothesis, generating Chi-square(11) statistics of 69, 88, and 62 for the drug coverage cap, the generic copayment level, and the brand copayment level, respectively. These results implied that differences in premium status should be accounted for in a statistical model of plans' benefit decisions. Therefore, the use of switching models was appropriate.

The first step in our switching models was the estimation of Equation (1) by probit methods (Table 3). The results show, as expected, that higher payment rates reduce the likelihood of charging nonzero premiums. Also as expected, higher industry concentration, higher income, and higher historical costs increase the probability of charging a premium. Higher PIP-DCG risk scores were associated with lower probability of charging a premium, consistent with the argument that, in the context of these models at least, the risk score should be interpreted as a measure of hospital utilization (and therefore cost-saving potential) rather than as a measure of underlying cost. Likewise, higher numbers of physicians and higher proportions of elderly residents were associated with lower probability of charging a premium, consistent with the bargaining power and information exchange interpretations discussed above. Most surprisingly, the indicator variable reflecting the presence of at least one premium-charging plan in the county in

2000 was associated with a lower probability of charging a premium in 2001.[6] Finally, the indicator for March was associated with a lower probability of charging a premium, suggesting that, apart from the effects on payment rates, BIPA may have increased the level of commitment of plans to the M+C market.

The first tests of our prediction about inefficient benefits can be found in the first row of Table 4, which reports switching regression results for generic and brand name prescription drug copayments as well as outpatient prescription drug benefit caps. As predicted, higher payment rates were more likely to reduce copayment levels among zero premium plans relative to plans charging nonzero premiums (the comparisons of elasticities of copayments with respect to payment rates were -0.25 vs. 0.37 and –0.68 vs. 0.18 for generic and brand name, respectively), and these differences were statistically significant. Consistent with these results, point estimates for the relationship between prescription drug benefit caps and payment rates were greater for zero premium plans than for plans that charge premiums (the elasticities were 0.26 vs. –0.41), although this difference was not significant.

Beyond these findings, only a few other variables had significant effects and patterns across specifications were not consistent. Industry concentration (Herfindahl index) was associated with slightly lower generic and brand name copayments and with substantially lower prescription drug benefit caps in the nonzero premium regimes but had insignificant effects otherwise. Similarly, higher risk scores and lower historical costs were associated with higher generic and brand name copayments in the nonzero premium regimes but had generally insignificant effects otherwise.

Discussion and Conclusions

In this paper we investigated whether empirical evidence supports the theoretical prediction that zero premium health plans in the M+C program are likely to offer some inefficient benefits (benefits that they would not have offered if they could have offered premium rebates instead). We took advantage of a natural experiment that occurred when Congress passed the Benefits Improvement and Protection Act in December of 2000. The passage of this law so late in the year resulted in two adjustments to payment rates, and to premiums and benefits in response, that were separated by only a few months. By choosing to focus attention on data from January and March of 2001, we minimized intertemporal variation in the cost of providing coverage to beneficiaries (a quantity that is notoriously hard to measure and has been changing rapidly) while preserving BIPA-induced variation in payment rates, premiums, and benefits. Consequently, these data presented an unusual opportunity to study the relationships between the premium and benefit decisions of plans and the payment rates and levels of competition they face, without the potentially confounding influence of unobserved changes in cost.

We estimated models of plan behavior with respect to three benefit variables: copayment levels for generic drugs, copayments for brand name drugs, and the overall cap on outpatient prescription drug coverage. Hausman tests revealed that plan behavior differed significantly by premium status and prompted us to re-estimate our equations using endogenous switching models. Our findings indicated that benefits in zero premium plans were more sensitive to changes in payment rates relative to plans that charged nonzero premiums. These results strongly suggest that the current Medicare

policy that prevents premium rebates effectively forces health plans to offer benefits that plan administrators do not believe are valued by enrollees at or above their cost. CMS is currently planning to allow M+C plans to offer premium rebates to beneficiaries, starting in 2003, although rebates will be taxed at a rate of 20 percent (Feldman et al., 2001). Our findings indicate that this change can be expected to result in better value for society in general and Medicare beneficiaries in particular. However, if the 20 percent tax prevents plans from actually offering rebates, the potential efficiency gain will be lost.

Although our results are quite robust, at least two cautions apply. First, the strength of this analysis comes from its tight focus on a particular period in time, but this is also a weakness. The M+C program was in substantial turmoil during the first few months of 2001 and relationships observed during that time might not be as generalizable as they would be if a longer period of study could have been used. Second, the inability to directly observe the cost of providing coverage makes this type of analysis challenging, even under favorable conditions like those following the passage of BIPA. It will be difficult to confirm these results with future data without a method for observing and measuring this cost.

Figure 1

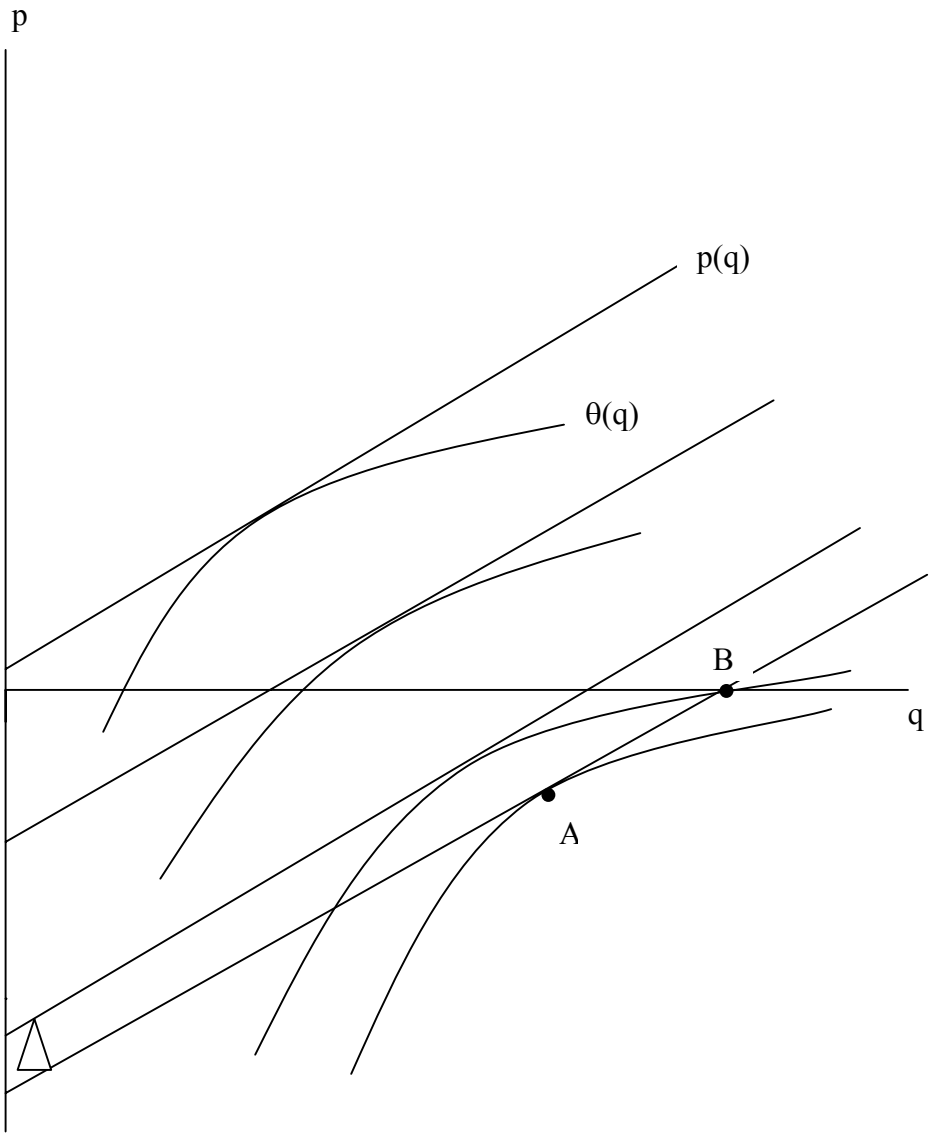

Table 1

Mean values and sample sizes for benefit variables by premium status

Benefit	*Zero premium observations*		*Nonzero premium observations*	
	Mean	*Obs*[(a)]	*Mean*	*Obs*[(a)]
Generic copayment	$9.56	423	$8.17	404
Brand name copayment	$23.68	382	$17.17	382
Outpatient prescription drug coverage cap	$686.40	228	$827.19	311

(a) To match the samples in Table 4, these samples exclude plan-counties lacking drug benefits, variation in premium status, or valid data for the dependent variables.

Table 2

Hausman test results for differences in plan decisions by premium status

Benefit	*Drug coverage cap*	*Generic copayment*	*Brand copayment*
Chi-square(11)	28	48	34
p-value	0.003	0.000	0.0003

(a) Hausman tests were conducted by estimating benefit models on the entire sample and on a sample restricted to zero premium observations. Since results from the two samples were significantly different in all three cases, these tests indicate that zero premium plans behaved differently than plans in general.

Table 3
Probit results:[(a)] monthly premium > $0

Variable	*Monthly Premium>0*	
	Coefficient Value (Standard Error)	*Marginal Probability Effect*
$payment_t^c$	-0.021***	-0.8%[(b)]
	(0.0026)	
$risk^c$	-16.9***	-61%[(c)]
	(3.56)	
Adjacent	-0.43	-16%[(d)]
	(0.35)	
Income (in thousands)	0.082***	3%[(b)]
	(0.023)	
Proportion population 65+	-10.2**	-37%[(c)]
	(3.17)	
Lagged Herfindahl index	1.16*	4%[(c)]
	(0.59)	
Beds per 100 persons	1.1	41%[(b)]
	(0.74)	
Part A per capita spending	0.0028***	0.1%[(b)]
	(0.0004)	
Physicians per 100 persons	-3.6***	-132%[(b)]
	(1.1)	
March	-0.40**	-14%[(b)]
	(0.16)	
Lagged other > 0	-0.94***	-35%[(b)]
	(0.28)	
	N=403	
	Pseudo R^2 =0.38	

(a) Although not listed here, the probit also includes a variable for each plan to control for plan-fixed effects as described in the text.
(b) Represents the change in probability due to a one unit increase in this independent variable.
(c) Represents the change in probability due to a 10 percentage point increase in this independent variable which ranges over [0,1].
(d) Represents the change in probability due to a change from 0 to 1 in this binary independent variable.

* indicates significance at the 5% level.
** indicates significance at the 1% level.
*** indicates significance at the 0.1% level

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Table 4
Switching regression results:[a] generic copayment, brand name copayment, and outpatient prescription drug benefit cap regressions

	Generic Copayment		*Brand Copayment*		*Outpatient Prescription Drug Benefit Cap*	
Variable	*Zero Premium Coefficient Value (Standard Error)* [b]	*Nonzero Premium Coefficient Value (Standard Error)*	*Zero Premium Coefficient Value (Standard Error)*	*Nonzero Premium Coefficient Value (Standard Error)*	*Zero Premium Coefficient Value (Standard Error)*	*Nonzero Premium Coefficient Value (Standard Error)*
$payment_t^c$	-0.0045	0.0066*	-0.029***	0.0076	0.39	-0.63
	(0.0030)	(0.0027)	(0.0058)	(0.0053)	(0.37)	(0.48)
$risk^c$	-0.25	7.0***	-7.3	13***	-241	212
	(1.7)	(1.4)	(4.0)	(2.7)	(235)	(282)
Adjacent	-0.34	0.37**	-1.3**	0.46	30	-2.6
	(0.18)	(0.14)	(0.42)	(0.47)	(45)	(41)
Income (in thousands)	0.0077	-0.0028	0.024	0.0074	0.82	-0.35
	(0.012)	(0.010)	(0.032)	(0.018)	(0.90)	(1.7)
Proportion population 65+	5.8	-6.4	31***	1.7	126	-662
	(4.0)	(3.7)	(9.0)	(4.0)	(295)	(443)
Lagged Herfindahl index	-0.37	-0.81**	-1.0	-1.9*	80	-218*
	(0.36)	(0.28)	(1.2)	(0.87)	(67)	(100)
Beds per 100 persons	-0.046	-0.19	0.13	-0.10	-5.7	-10
	(0.11)	(0.21)	(0.41)	(0.83)	(21)	(36)
Part A per capita spending	0.00005	-0.00093***	0.0016*	-0.0018**	0.039	0.058
	(0.0003)	(0.00035)	(0.0007)	(0.0008)	(0.053)	(0.059)
Physicians per 100 persons	-0.75	0.50*	-2.7*	0.079	37	-60
	(0.45)	(0.23)	(1.2)	(0.72)	(43)	(72)

Table 4
Switching regression results:[a] generic copayment, brand name copayment, and outpatient prescription drug benefit cap regressions

	Generic Copayment		*Brand Copayment*		*Outpatient Prescription Drug Benefit Cap*	
Variable	*Zero Premium Coefficient Value (Standard Error)* [b]	*Nonzero Premium Coefficient Value (Standard Error)*	*Zero Premium Coefficient Value (Standard Error)*	*Nonzero Premium Coefficient Value (Standard Error)*	*Zero Premium Coefficient Value (Standard Error)*	*Nonzero Premium Coefficient Value (Standard Error)*
March	0.060	0.10	-0.10	-0.15	-6.0	0.31
	(0.27)	(0.23)	(0.42)	(0.29)	(45)	(42)
Lagged other average copay	-0.077	-0.10	0.15	0.041	40	45
or any cap over $800 dummy	(0.096)	(0.06)	(0.14)	(0.045)	(23)	(34)
Inverse Mills Ratio[c]	0.76	-0.25	1.2	-0.40	-152	24
	(1.8)	(0.95)	(2.7)	(0.95)	(346)	(523)
	N=423 R^2=0.99	N=404 R^2=0.99	N=382 R^2=0.99	N=382 R^2=0.99	N=228 R^2=0.99	N=311 R^2=0.98

(a) Although not listed here, regressions also include a variable for each plan to control for plan-fixed effects as described in the text.
(b) Standard errors are computed by bootstrapping with 400 repetitions.
(c) The inverse Mills ratio is calculated to be the expected value of the disturbance term from the first stage (Equation 1), conditional on premium=0 or premium>0, using the coefficients reported in Table 3.

* indicates significance at the 5% level.
** indicates significance at the 1% level.
*** indicates significance at the 0.1% level.

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Notes

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[1] The Herfindahl index is defined to be the sum of squared market shares in a particular industry or market. In this case, we use the sum of squared market shares of M+C plans in each county.

[2] Not only is it intuitively appropriate to give smaller plans less weight, this weighting also serves as a correction for possible heteroscedasticity in the OLS models. However, because of uncertainty in the assignment of enrollments (see note 4), we also estimated our models without weights, producing qualitatively similar results.

[3] This is a Hausman test because we compared an efficient estimator under the null (full sample) to an inefficient but consistent estimator (restricted sample).

[4] In some cases (about 20% of plan-counties in 1999), plans offered more than one package of benefits in a county. Since the State/County/Plan files contain only one enrollment number for each plan in each county, some assignment rule was necessary. Following Gold (2001), we assigned each plan-county's enrollment to the package of benefits with the lowest premium and (in case of ties) the most generous drug benefits. Our qualitative findings, however, do not depend on this assignment (see note 2).

[5] For example, a model for dental coverage would only be identified for plans that had zero premium and nonzero premium observations, and had observations with and without dental coverage *within* each of those groups. This second requirement is much more likely to be satisfied with continuous variables.

[6] This result is a consequence of the need to drop plans without variation in the dependent variable across counties or across time (e.g., those that charged a premium in all counties in 2000 and continued to do so in 2001) to identify the probit. This sample restriction is required if plan-level fixed effects are included in the specification. Note that if plan effects are dropped and the sample is expanded, the sign of this coefficient is reversed.